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Social ties at work and effort choice: Experimental evidence from Tanzania ☆

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ABSTRACT

Many firms hire workers via social networks. Whether workers who are socially connected to their employers exert more effort on the job is an unsettled debate. We address this question through a novel experiment with small-business owners in Tanzania. Participants are paired with a worker who conducts a real-effort task, and receive a payoff that depends on the worker's effort. Some business owners are randomly paired with workers they know, while others are paired with strangers. We find that being connected to one's employer does not affect workers' effort on average, but increases the effort of workers without children. Our results are consistent with workers having an altruistic drive in exerting effort when they work for someone they know, which fades away when their valuation of private income becomes stronger.

1. Introduction

Many workers find jobs through social networks, a fact that has been documented both in high-income countries (Granovetter, 1995; Munshi, 2003; Ioannides and Datcher Loury, 2004; Kramarz and Skans, 2014) and in low-income ones (Wahba and Zenou, 2005; Munshi and Rosenzweig, 2006; Beaman and Magruder, 2012; Witte, 2018). However, whether firms obtain *more productive* workers through this practice is still an open question. In general, hiring workers through social networks can affect worker productivity in at least two ways. First, it can help firms screen workers when skills are hard to observe and available signals are noisy. Such screening activity has been documented in a variety of settings (Montgomery, 1991; Munshi, 2003; Dustmann et al., 2016). Second, being socially connected to the employer may increase workers' effort conditional on their underlying skills and characteristics. This second effect could be driven, for instance, by greater altruism towards an employer the worker knows or increased pressure and fear of punishment (Kugler, 2003).[1]

We focus on workers' effort choice in the presence vs absence of social ties with their employer, and shed light on the underlying mechanisms. Specifically, we test whether altruistic motives or fear of retaliation drive workers' choice of effort when they work for an employer they know vs one they do not know. We do so through a novel lab experiment conducted with actual employers and workers in Tanzania, which cleanly tests whether working for an employer with whom a worker shares social ties affects the effort choice of the worker. To our knowledge, this is the first study that experimentally varies the connection between employers and workers, and investigates how this affects effort choice.

We conducted our experiment with 313 real-life small and micro-entrepreneurs, sampled from multiple markets and business hubs across Dar es Salaam, Tanzania. Each entrepreneur was asked to bring along one further person they knew (henceforth the *worker*). This could be one of their existing employees or a person they would consider hiring if necessary. Each worker was matched with one of the entrepreneurs during the experiment and had to conduct a real-effort task, which consisted of sorting beans by colour. For each gram of sorted beans both the worker and the entrepreneur received a monetary payment

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[1] A worker who is socially connected to an employer may also exert *lower* effort if social ties reduce the risk of retaliation (e.g., layoff) by the employer (Ponzo and Scoppa, 2010) or increase rewards for equal output. Indeed, workers who find jobs via social contacts have been shown to stay longer in these jobs (Datcher, 1982; Simon and Warner, 1992; Loury, 2006).

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at the end of the experiment. Crucially, one-half of the entrepreneur–worker pairs that arrived together at the lab sessions were randomly selected to *remain matched* with each other for the real-effort task, while among the other half the workers and entrepreneurs were *randomly re-matched*. Given that the pay-offs for both entrepreneurs and workers depend solely on the effort of the worker, and given that workers and entrepreneurs have in expectations the same characteristics in the two groups, we can cleanly isolate the effect of working for a connected entrepreneur on workers' effort choice.

We also implemented a variation of the basic design with a sub-sample of our entrepreneur–worker pairs to test whether fear of retaliation by socially connected employers is a mechanism driving worker effort choice. In this variation, we (truthfully) informed the workers that their identity would not be revealed to the entrepreneur they were matched with, regardless of whether it was the one who brought them to the sessions or a stranger. Not knowing the identity of the worker limits the possibility of retaliation by the entrepreneur.

We find that workers who work for an employer they know exert higher effort if they do not have children, while we do not find a similar effect among workers with children. Over the full sample, we do not find evidence that being connected to one's employer increases workers' effort. The effect among the childless seems not to be driven by fear of retaliation, but rather by altruistic motives. This is consistent with altruism being crowded out by a higher marginal valuation of private income when one needs to provide for children. We also do not find evidence of workers receiving higher rewards for equal output if they share social ties with their employer. We present a brief theoretical model to formalize our hypotheses. Importantly, the model does not require effort to be unobservable, as is typical in models of moral hazard. In our model, like in our experimental task, effort maps directly into output and can therefore be assumed observable. The observability of effort allows us to focus on the extent to which the (socially) optimal level of effort varies with the presence of social ties between workers and employers.

Our work contributes to a growing literature on the effect of social connections in the workplace. The existing evidence on how such connections impact workers' effort is mixed. Heath (2018) and Dhillon et al. (2021) find that social ties increase workers' effort in Bangladesh and India, respectively. On the other hand, Pallais and Sands (2016) and Burks et al. (2015) find that workers hired through referrals from existing workers are better selected, but do not exert more effort conditional on type. A broader literature has studied whether working alongside friends in the workplace affects workers' effort, finding negative effects (Park, 2019), no effects (Brune et al., 2022), effects that depend on relative abilities of the friends (Bandiera et al., 2010), or peer effects, in that temporarily higher effort of friends increases own effort (Falk and Ichino, 2006; Mas and Moretti, 2009; Herbst and Mas, 2015). Interestingly, these effects often appear driven by mechanisms other than altruism, such as preference for conformity (Bandiera et al., 2010) or peer pressure (Heath, 2018; Mas and Moretti, 2009), in line with our evidence on the presence of altruism in only specific sub-sets of our sample. Also DellaVigna and Pope (2017) find only a muted role of altruism in affecting effort in a real-effort task in an online experiment.

While the existing literature on the effects of social connections at work is largely based on observational studies, we implement a novel experiment that randomly varies the degree of social connectedness between employers and workers. Only Pallais and Sands (2016) conducted an experiment before, which, however, did not directly vary the presence of social ties in the workplace. They instead work with a firm that had recently hired a number of new workers referred by existing employees and inform a randomly selected group of these workers that the person who referred them was being informed about their performance. The treatment, therefore, consisted of making existing connections more salient. We, instead, create and sever matches experimentally. In addition, while the design of Pallais and Sands (2016) concentrates on the role of social pressure, our setup allows us to investigate other mechanisms such as altruism. The above studies also tend to focus on social ties *among workers*, while ours concentrates on social ties *between workers and employers*. Worker-to-worker relationships likely raise additional complexities, such as career concerns (Gibbons and Murphy, 1992) or the structure of payment schemes (Bandiera et al., 2009). Such complexities may have contributed to the diverging results found in the above-mentioned papers.[2] Furthermore, our heterogeneous results with respect to having children contribute to the literature on the effects of parenthood on labor market outcomes (Lundborg et al., 2017; Cortés and Pan, 2023).

We also contribute to the literature investigating whether hiring within social and kinship networks in low-income countries is efficient or rather a friction constraining growth. The latter may be the case if social norms force firms to hire from kin- or friendship networks.[3] On the other hand, hiring from such networks could be an optimal response to lack of trust in workers from outside the network (Bloom et al., 2012; Bertrand and Schoar, 2006). Caria and Falco (2022) experimentally document such lack of trust in Ghana and show that it can constrain firm growth. We investigate another potential reason for hiring within social networks: connected workers may exert higher effort, all else equal.

Finally, our results speak to the larger literature studying constraints to firm growth in low-income countries. The focus of such studies ranges from access to capital (De Mel et al., 2008; Banerjee et al., 2015), to informality (De Mel et al., 2013; De Soto, 1989), lack of business knowledge (Karlan and Valdivia, 2011; McKenzie, 2021), or lack of networks and mentoring opportunities (Fafchamps and Quinn, 2018; Brooks et al., 2018). A more recent literature has placed the spotlight on the misallocation of talent and on the challenge of finding good matches when information frictions are significant (Abebe et al., 2020, 2021; Alfonsi et al., 2020; Hardy and McCasland, 2020). We study whether firms face a trade-off between hiring from a limited pool of socially connected workers, who may not be the best matches in terms of skills but may exert more effort on the job. Disentangling the two mechanisms has so far proved difficult without a suitable experimental design. We bridge this gap by isolating and testing the effort channel with a novel experiment.

The remainder of the paper is structured as follows. Section 2 describes the experimental design and our sample. Section 3 outlines a simple theoretical framework to guide the interpretation of our results. Section 4 presents the results. Section 5 concludes.

2. Experimental design

We invited 320 small business owners (henceforth the "entrepreneurs") from the city of Dar es Salaam to take part in an experimental lab session conducted at the University of Dar es Salaam. Almost all of them, 313 in total, attended the sessions. Each entrepreneur was asked to bring one other person of their choice to the session (the "worker"). This should be a person who either worked at their business, or could be employed if needed. Both entrepreneurs and

[2] A large literature has studied the effects of pay and incentives schemes on worker's effort, with effects that depend very much on the studies' contexts. A few seminal papers showed that tying pay to worker output increases output (Lazear, 2000; Shearer, 2004). However, while increasing worker output, this may not increase firm profits (Freeman and Kleiner, 2005), and may even crowd out intrinsic worker motivation (Bénabou and Tirole, 2003) or rational decision making (Achtziger et al., 2015).

[3] We are not aware of research that has directly tested this channel. However, research from low-income settings has documented pressure within kinship networks to share income (Boltz et al., 2019; Baland et al., 2011), and providing jobs is an important way to distribute income. Also, Bertrand and Schoar (2006) show that firms in countries with more deeply ingrained "family values" have fewer workers.

workers were offered a 3,000 TZS show-up fee for participating in the lab experiment (around 1.30 USD).[4]

2.1. Baseline setup

The experiment is centred around a real-effort task that takes place in the lab. The task is very simple and requires no particular skill. Starting from a bag with three different kinds of beans, workers need to sort the different types of beans into separate batches. Workers have eight minutes to sort as many beans as they can. For every gram of correctly sorted beans, they are paid two Tanzanian Shillings (TZS), in addition to a fixed amount of 700 TZS (ca. 0.30 USD), which is independent of performance. On average, workers sort around 350 grams of beans in 8 minutes and thus earn another 700 TZS. The chosen task has been used in previous experimental work (e.g., Caria and Falco, 2022). The objective is not to replicate a specific real-life job, but rather to provide a clean setting to isolate the role of effort when ability plays a very limited role. In this respect, the chosen task can be considered a stylized representation of basic manual tasks that exist in many industries and require workers to apply steady effort in order to attain a simple objective.

Before the first round of the game starts, participants have a few minutes to acquaint themselves with the task and the materials. Workers then play three consecutive rounds of the game (each lasting 8 minutes). For the current analysis, we only use data from the first two rounds.[5] In Round 1, workers are simply asked to perform the task for themselves, without an employer, receiving two TZS for every gram plus the fixed amount of 700 TZS. This allows us to obtain a measure of their baseline productivity at the task. In Round 2, workers repeat the task but are now matched with an entrepreneur, who receives four TZS for every gram the worker produces, while the worker continues to receive two TZS for every gram plus the fixed amount of 700 TZS.[6] In the baseline version of Round 2, the entrepreneur remains completely passive. The work is done by the worker and the entrepreneur has no decisions to make. He or she only affects output to the extent that his or her identity, which is revealed to the worker, affects the worker's effort choice. The design intends to replicate real-world situations in which an entrepreneur hires a worker to conduct a task, and profits depend on the effort of the worker, who receives a piece rate. Such a payment scheme is common across a range of sectors and occupations in Tanzania.

The key experimental variation is that half of the entrepreneur–worker pairs that arrived at the sessions were randomly chosen to remain matched with each other for the session. Among the remaining pairs, the workers and entrepreneurs were randomly re-matched. Given that entrepreneurs were sampled from many different locations across Dar es Salaam, a city of more than six million inhabitants, it is highly unlikely that entrepreneurs and workers in re-matched pairs knew each other. We refer to workers matched with the entrepreneur who brought them to the sessions as those who "remained matched", and to the others as "re-matched". This experimental design allows us to identify the impact of being randomly assigned to work for an entrepreneur with whom one is connected versus one with whom the worker does not share a prior connection, all else equal. We further collected information on the exact nature of the relationship between the employers and the workers they brought to the sessions (worker/boss, friend, kin, . . .), which allows us to test for heterogeneous effects by type of relationship between workers and entrepreneurs.

2.2. Anonymous worker variation

In one quarter of the experimental sessions, we *hide the identity of the worker from the entrepreneur* to whom the worker was matched. In this variation, while workers are told whom they are matched with, the entrepreneur does not know whether s/he has been matched with the worker they brought to the sessions or with another randomly selected worker. The workers are informed that their matched entrepreneur is not informed about their identity. This allows us to test whether fear of *retaliation* plays a role in workers' effort choice. By not informing entrepreneurs about the identity of their matched workers, we make it unlikely that the entrepreneur can retaliate against the worker for low output.[7] If retaliation is a mechanism that drives different effort levels of socially connected workers, we would expect this effect to be smaller if the identity of the worker is hidden from his or her matched entrepreneur.

We also implemented two further variations of the design in further subsets of experimental sessions, to test if socially connected workers are *rewarded differently for the same output*. In one variation, we allowed entrepreneurs to *tip* their matched worker after observing their output in order to study if the output of socially connected workers is rewarded differently ex-post, and if anticipating potentially different ex-post rewards affects workers' effort choice. In the second variation, entrepreneurs were asked before Round 2 to choose how to split the combined piece rate of 6 TZS per gram between themselves and the worker, to test whether they choose different piece rates for connected and non-connected workers, thus offering them different ex-ante rewards. We do not find evidence of either form of different rewards for socially connected workers, as shown in more detail in Appendix A.

2.3. Sampling

We conducted the experiment with a sample of 313 entrepreneurs who attended the lab sessions out of an initial 320 who accepted our invitation. Each entrepreneur brought along one worker, as described above. The sampling of the entrepreneurs was conducted using a random walk methodology in selected retail and service areas of Dar es Salaam, as well as in a number of manufacturing clusters. From a randomly selected starting point, e.g. a prominent intersection, the research assistants started walking in a randomly drawn direction and invited the owner (or manager, if the owner was not present) of every sixth business to take part in the experiment. Upon arriving at another intersection or road fork, a randomly selected direction was drawn to continue the random walk. The experiment was implemented in 32 sessions with 10 "entrepreneurs" and 10 "workers" each. Within each

[4] The experimental scripts can be found in Appendix D.

[5] Round 3 was originally designed to address ancillary research questions that are not central to the main hypothesis being investigated. However, due to implementation problems in the random re-matching of workers and entrepreneurs in the third round, the data from that round cannot be used to isolate the impact of the additional treatments in Round 3. It can however be left out without affecting the results from the first two rounds and their interpretation, since participants were only informed about the game of Round 3 after Round 2 was concluded.

[6] The 700 TZS were added to the per-round pay-off of the workers in order to be able to pay a higher per-gram piece rate to entrepreneurs than workers while keeping the total pay-offs for workers and entrepreneurs roughly equivalent (for equity considerations). The higher piece rate for entrepreneurs was meant to make the effect of workers' effort on entrepreneurs' payoff more salient to workers. The fixed payment also mimics real world contracts where employees normally obtain some fixed payment regardless of effort. As mentioned above, both entrepreneurs and workers also received a 3000 TZS show-up fee for participating in the lab experiment.

[7] Entrepreneurs may still assume that they are matched with their connected worker with a certain probability given the set-up of the lab sessions, and may decide that it is optimal to punish the worker they brought along to the session anyway in any case of low output. However, even if the probability of punishment for low output may not be zero when the worker's identity is experimentally concealed, it can nevertheless be assumed to be lower than when it is not concealed.

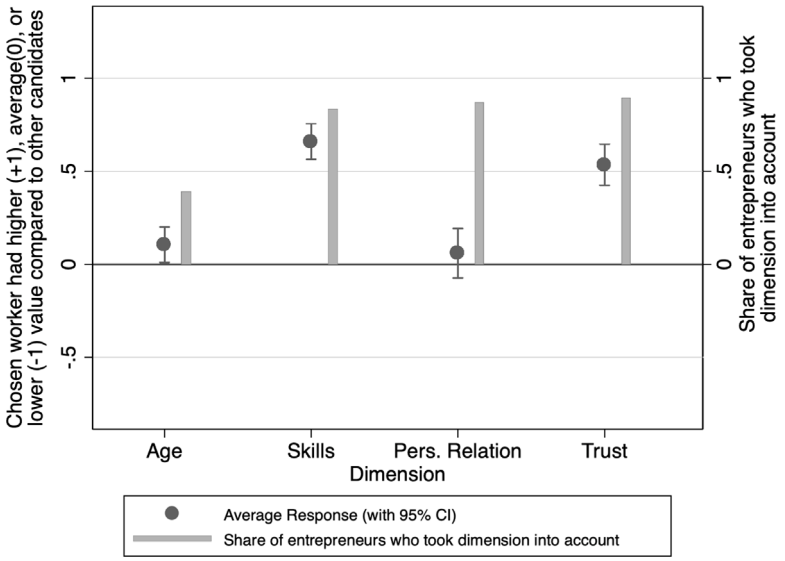

Fig. 1. Selection of Workers by Entrepreneurs.
Notes: The figure shows how the workers selected by the entrepreneurs for the sessions compared to the pool of workers they could have chosen from. The dots (with 95% confidence intervals) capture the average answer to the following question: "Compared to a set of people you could have considered bringing with you to the session, how would you describe the one you brought in terms of age/of knowledge about your business/personal relationship/trust?" Responses were coded as 1 for "higher/stronger than the typical candidate", −1 for "lower/weaker than the typical candidate", and 0 for "similar to the typical candidate". The bars indicate the share of respondents who reported that they took the dimension into account when selecting whom to bring to the session.

session, half of the entrepreneur–worker pairs were randomly chosen for re-matching. The entrepreneurs and workers in those pairs were randomly re-matched with other workers and entrepreneurs within the same session.[8]

2.4. Selection of workers by entrepreneurs

After the experiment, we re-surveyed 200 entrepreneurs to shed light on how they selected the workers they brought to the experimental sessions. We asked the entrepreneurs whether "among the candidates they considered, the selected person scored higher or lower along the following four dimensions": age, skills for the work in their business, strength of personal ties to the entrepreneur, and trust by the entrepreneur. As shown in Fig. 1, the entrepreneurs reported that the selected candidates were on average among the more skilled and more trusted candidates the entrepreneur could consider, while they did not report that these were candidates to whom they had particularly strong social ties. There seems to have been a slight positive selection with respect to age, but only 40 percent of the re-surveyed entrepreneurs reported having taken age into consideration, while around 80 percent reported having taken the other three dimensions into consideration (as shown by the bars in Fig. 1). Thus, the sample of entrepreneur–worker pairs does not appear to be selected based on the strength of personal relationships, suggesting that our results on the effects of severing social ties in the workplace should not be read as pertaining to particularly strong or weak ties in the workplace, and may therefore have high external validity.

2.5. Sample and experimental balance

Table 1 shows additional summary statistics for the entrepreneurs who participated in the sessions, as well as for the workers they brought along. As shown in Column 1 of the table, 58.5 percent of the entrepreneurs are female, they are on average 40.2 years old, have on average 2.1 children, went to school for 8.5 years, and 51 percent of them are Muslim while the rest are Christian. Workers are 60 percent female. They are, on average, 33.8 years old, they have 1.9 children, attended school for 8.3 years, and 54 percent are Muslim. Finally, entrepreneurs have on average 0.95 (full-time) employees in their real-life businesses, with eight entrepreneurs having more than three (the maximum being seven employees).

Looking at the entrepreneurs who were randomly chosen to remain matched with the workers they brought, versus those who were not, Column 2 of Table 1 shows that the two groups are balanced. Only one of seventeen observable characteristics at our disposal shows a statistically significant difference at the 1 percent level (a second one only shows significance at the 10 percent level). Looking at the workers, we find a difference between the two groups in only one variable, and the difference is only significant at the 10 percent level (Column 4 of Table 1). We also reject the hypothesis that all the variables are jointly different between the two groups, for both entrepreneurs and workers, using F-tests.

2.6. Baseline evidence on hiring through social connections in the study population

Hiring from kinship or close friendship networks is common among the entrepreneurs that participated in our study: 65 percent referred to the worker they brought along as a "family member" or "friend", while 67 of workers reported the same about the employer who brought them along. 37 percent of the entrepreneurs agreed with the statement that socially connected workers exert more effort, while 52 percent disagreed (the rest neither agreed nor disagreed). 35 percent agreed that family members are less likely to "steal" from the business, and 47% agreed that if one spends salary on a worker, it is best to spend it on a family member.[9] On the worker side, 40 percent agreed that an employer with whom one shares social ties will put less pressure on one

[8] We aimed to have 10 entrepreneurs and their workers in each session. In some sessions, not all invited pairs turned up, resulting in six sessions with 9 instead of 10 pairs, and one with 8 pairs. Finally, one session had 11 pairs. In sessions with odd numbers of pairs, the pool of pairs to be re-matched contained one extra pair than the pool of pairs that stayed together.

[9] 27 percent agreed with both of the first two statements and 20 percent with all three.

Table 1
Summary Statistics & Balance.

	(1) Entrepr. matched w. own Worker (Mean)	(2) Entrepr. matched w. own - unknown W. (Diff)	(3) Worker matched w. own Entrepr. (Mean)	(4) Worker matched w. own - unknown Entr. (Diff)
Female	0.608	0.045	0.634	0.053
	(0.490)	(0.056)	(0.483)	(0.055)
Age	39.654	−1.140	33.529	−0.464
	(11.504)	(1.287)	(9.712)	(1.190)
Lives in Dar Es Salam	15.497	−1.366	13.484	−0.616
	(13.561)	(1.672)	(12.069)	(1.429)
Head household	0.654	−0.009	0.562	−0.032
	(0.477)	(0.054)	(0.498)	(0.056)
Married	0.660	0.066	0.536	0.055
	(0.475)	(0.055)	(0.526)	(0.059)
Number children	2.033	−0.130	1.843	−0.038
	(1.664)	(0.197)	(1.544)	(0.195)
Head household	0.654	−0.009	0.562	−0.032
	(0.477)	(0.054)	(0.498)	(0.056)
Household size	2.922	0.009	2.732	0.095
	(1.848)	(0.203)	(2.033)	(0.204)
Years schooling	8.451	−0.030	8.612	0.593
	(2.900)	(0.301)	(2.555)	(0.306)*
Pay taxes Y/N	0.562	−0.025	0.458	0.026
	(0.498)	(0.056)	(0.500)	(0.056)
Permanent employee Y/N	0.131	0.049	0.386	−0.027
	(0.338)	(0.035)	(0.488)	(0.056)
Monthly Inc. (USD)	200.220	16.954	106.223	−12.624
	(353.923)	(34.782)	(252.408)	(27.136)
Musl.(1) or Christ.(0)	0.431	−0.162	0.533	−0.011
	(0.497)	(0.056)***	(0.501)	(0.057)
Literate Y/N	0.935	0.060	0.902	0.046
	(0.248)	(0.033)*	(0.298)	(0.037)
Social Conn. to Partner	0.654	0.004	0.660	0.023
	(0.477)	(0.054)	(0.475)	(0.054)
Actual Worker for Boss	0.680	0.017	0.680	0.017
	(0.468)	(0.053)	(0.468)	(0.053)
Nbr. full time employees	0.915	−0.060	–	–
	(1.057)	(0.115)		
Output Round 1	–	–	326.484	−10.573
			(64.815)	(7.104)
Observations	153	313	153	313

Notes: The table summarizes key observable characteristics of the 313 entrepreneurs that participated in the sessions (Columns 1–2), and the 313 workers they brought along (Columns 3–4). Columns 1 and 3 show the average values of the observed variables for the 153 entrepreneurs and workers who were randomly selected to remain matched with the worker or entrepreneur with whom they arrived at the sessions, respectively. Columns 2 and 4 show differences in these variables, for entrepreneurs and workers, respectively, between those randomly selected to remain matched with the person they came to the sessions with and those who were re-matched with a stranger. An F-test of whether all variables jointly predict treatment allocation produces a p-value of 22 percent for entrepreneurs and 66 percent for workers. * $p < .1$; ** $p < .05$; *** $p < .01$.

to work hard, 39 percent agreed that employers with whom one shares ties are less likely to cheat on wage payments, and 26 percent agreed that such employers will pay more for the same work. Meanwhile, only 10 percent of employers state that they would pay a friend or family member more for the same work. Overall, this descriptive evidence paints a rich picture with several potential mechanisms at play. We next present a simple model that distills these qualitative patterns and helps us to clarify what are the channels we aim to test with our data.

3. Conceptual framework

We lay out a simple model to fix ideas on how the presence (or absence) of social ties between workers and employers can affect workers' effort choice. In addition to standard monetary incentives such as piece rates, the model features altruism towards one's employer, potential retaliation by the employer for low output, and the interplay of these elements.

We assume the following utility function for a worker, who has to decide how much effort e to expend on a task, which, given the simplicity of our experimental real-effort task, we equate to the output of the task:

$$U_i = (p + \alpha(s))\, e - e^{\gamma} \tag{1}$$

The parameter p captures the *private* benefits to a worker per unit of his or her output (i.e. in the form of income), while $\alpha(s)$ captures *social* benefits of the output to the worker, i.e. whether s/he derives utility from his/her output benefiting other persons, for example, the employer (in other words, s/he is altruistic). We let s denote the strengths of social ties to this other person, and assume that $\alpha(s)$ is weakly increasing in the strength of the social ties, $\delta\alpha(s)/\delta s \geq 0$. Finally, we make the standard assumption that the marginal cost of effort is increasing with effort, as captured by e^{γ}, $\gamma > 1$ (Lazear and Oyer, 2012). The optimal effort e^* of the worker is derived from maximizing utility with respect to effort:

$$e^* = \left(\frac{p + \alpha(s)}{\gamma}\right)^{\frac{1}{\gamma - 1}} \tag{2}$$

A core test of our paper is whether $\delta\alpha(s)/\delta s$ is positive. If this is the case, the model predicts that also effort, and thus output, increases with social ties, given that $\delta e^*/\delta\alpha > 0$.

We can use the model to explore other predictions, for example, how the effect of social ties on optimal effort e^* depends on the strength of private incentives p. The cross derivative of Eq. (2) with respect to $a(s)$

and p is

$$\frac{\delta^2 e^*}{\delta a(s)\,\delta p} = \frac{2-\gamma}{\gamma^2(\gamma-1)^2}\left(\frac{p+\alpha(s)}{\gamma}\right)^{\frac{3-2\gamma}{\gamma-1}} \tag{3}$$

The cross-derivative becomes negative if the curvature of the effort cost curve is convex enough, i.e. if $\gamma > 2$. In that case, higher private benefits of output (a higher p) reduce the sensitivity of optimal effort e^* to variations in the strength of social benefits, as captured by $a(s)$.[10] Effort costs may indeed be very convex for our real effort task; at one point increasing the rate with which a worker can sort beans by color through more effort provision is limited by the physical attributes of the worker (i.e. the degree of dexterity of his/her hands). This aspect of the task may be more broadly representative of the simple manual jobs characterizing our setting.

We extend the model to incorporate retaliation by the employer for low output of the worker in Appendix C of the paper, as this extension requires more space. The main result is that under perfect information about the cost of effort to the worker, the retaliation channel increases the output of socially connected workers. Thus, both the altruism and retaliation channels operate in the same direction. However, if one adds more complexity to the model, i.e. imperfect information, these predictions become less sharp. We also extend the model in the second part of Appendix C to feature employers internalizing workers' effort costs, and show that it reduces the sensitivity of worker effort to social ties.

4. Results

In this section we present the main results from the experiment. Next to looking at average effects in the overall sample (Section 4.1), we seek to separate the retaliation channel from the altruism channel (Section 4.2), and investigate whether treatment effects vary along observed workers' characteristics (Section 4.3). We then provide results from a smaller follow-up experiment from the same setting that varied private piece rates, to test whether our results are valid beyond our limited sample (Section 4.4). We conclude by testing whether the specific type of social connections matters for the size of treatment effects (Section 4.5), and discussing evidence on potential additional mechanisms (Section 4.6).

4.1. Main results

We begin by showing basic comparisons of the output in grams of beans sorted by workers that "remained matched" with the entrepreneur who brought them to the experiment relative to those that did not. In the standard terminology of the experimental literature, "Remained Matched" is thus our core randomized treatment indicator variable. Column 1 of Table 2 shows that workers who were re-matched with a stranger sorted on average 356 grams of beans, while those that remained matched with the person who brought them sorted six grams less. The difference is small and not statistically significant. In percentage terms, this is a difference of 1.8 percent and our minimum detectable effect size (at the 5 percent level) is 5.3 percent (2.8 × coefficients' standard error). The effect size we estimate shrinks even further when we control for gender and the "baseline" output of the worker in Round 1, the practice round in which the worker produced only for his/her own gain (Column 2 of Table 2). The estimated effect of remaining matched is now a positive 2.25 grams, or 0.6 percent of the output produced by non-connected workers. The minimum detectable effect in this specification is a 3.3 percent difference. Thus, we can conclude that if working with a connected employer had economically meaningful effects on output, we would have detected them.[11]

So far, we assumed that all workers have social ties to the entrepreneurs who brought them to the session. This need not be the case. Some workers could be employees the entrepreneur hired from outside any social network, and with whom s/he shares a purely professional relationship. This could bias the estimated effect of social ties downwards. Since we asked workers and entrepreneurs about the nature of their relationship with the person with whom they arrived, we can test for this possibility. As already mentioned above, among both entrepreneurs and workers, about 65 percent state that the person they came with is an (extended) family member or friend, while the rest describe the person as a "colleague", "boss", or similar. In Column 3 of Table 2, we replicate the results from the previous column on the sample of the 203 workers who report that their relationship to the person who brought them along is one of friendship or kinship. The results remain qualitatively unchanged, with the treatment coefficient in this sub-sample being less than half a percent of the average control-group output.[12]

4.2. Retaliation: Anonymous workers

As discussed above, connected workers may not only work harder due to altruism but also for fear of retaliation. To test for this mechanism, in 8 out of 32 sessions we hide the identity of the worker to their matched entrepreneur. Thus, the entrepreneur does not know if the worker s/he is matched with is the worker whom s/he brought to the session, and is therefore less likely to retaliate for low effort.

We find that hiding the identity of the worker does not change our results. When we interact the dummy for "Remained Matched" with a dummy indicating that the game was played in a session in which workers' identities were concealed ("Anonymous Worker"), the interaction effect is small and not statistically significant (Column 4 of Table 2). As for the overall effect, this interaction effect does not qualitatively change when estimated on the restricted sample of workers who reported actual social ties (e.g., friendship, kinship) to the employer who brought them to the sessions (Column 5). Thus, consistent with a zero average treatment effect, we do not find evidence supporting the retaliation channel.

4.3. Heterogeneous effects: Parental status

We estimate a null effect of social incentives on productivity. This may be attributable either to the ineffectiveness of social incentives, or to a general lack of response of productivity to incentives in this context. Our approach is to evaluate whether there exist subsamples of respondents who are responsive to these social incentives using a Causal Random Forest (Wager and Athey, 2018), and then validate that similar subsamples are also responsive to private incentives using a new sample and separate experiment. If so, we will conclude that social incentives operate similarly to private incentives, and compare effect sizes to learn about the magnitude of those social incentives.

[10] Note that in that case higher private benefits also reduce the sensitivity of e^* to variations in private benefits p, given that also $\frac{\delta^2 e^*}{\delta^2 p} < 0$ if $\gamma > 2$.

[11] The estimated effects do not differ if we use the *difference* in output of a worker between Round 2 and 1 as the outcome, which captures directly the additional output that stems from adding the social incentive in Round 2 of the experiment. These additional results are shown in Appendix Table B.1.

[12] We also do not find evidence of treatment effects in other sub-groups that may be characterized by stronger social ties, such as workers who report being actual employees of the entrepreneur who brought them to the sessions (as opposed to just potential employees in case the need arose), or those who report knowing the employer for a longer time. These additional results are reported in Columns 1–4 of Table B.2. Similarly, we do not find significant treatment effects among the sub-samples of male or female workers, see Table B.3.

Table 2
Main Results.

Variables	(1) Output	(2) Output	(3) Output Act.Soc.Tie	(4) Output	(5) Output Act.Soc.Tie
Remained Matched	−6.403	1.997	−1.581	−0.394	−0.633
	(6.763)	(4.181)	(5.386)	(4.366)	(5.549)
Rem. Match × Anonymous Worker				9.852	−3.534
				(11.583)	(13.979)
Output Round 1		0.827***	0.863***	0.828***	0.863***
		(0.043)	(0.048)	(0.042)	(0.048)
Female		3.157	−1.927	3.297	−2.023
		(5.679)	(6.008)	(5.701)	(5.985)
Observations	313	313	203	313	203
Session FE	Yes	Yes	Yes	Yes	Yes
Mean re-matched	356.4	356.4	358.4	360	357.9

Notes: The table shows results from regressing workers' output in the real-effort task, as measured in grams of sorted beans, on the core independent variable of interest ("Remained Matched"), a dummy for whether the worker was randomly selected to remain matched with the entrepreneur s/he arrived at the experimental session with, as opposed to being randomly re-matched with another (stranger) entrepreneur from the session. All regressions except for that in Column 1 control for the gender and the output of the worker in a test round of the same task with no matched entrepreneur yet. Column 3 restricts the sample to workers who report that they have social connections of "kinship" or "friendship" to the entrepreneur they arrived with. Column 4 interacts the core variable of interest with a dummy indicating whether the identity of the worker is withheld from the matched entrepreneur. Column 5 replicates Column 4 on the sample of workers who report having social connections (e.g., friendship, kinship) to the entrepreneur they arrived with. All regressions control for session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

When we estimate the Causal Random Forest on our sample, we find that out of 18 potential variables used in the forest, having children has the highest statistical significance in predicting the conditional average treatment effects (CATEs) as estimated by the forest for each observation, with a p-value of 0.054, see Appendix Table B.5 for more details. While we are not sufficiently powered for the p-value to remain significant at standard levels after multiple hypotheses adjustment, this points to having children as the strongest predictor of CATEs.

Thus, we next interact our treatment indicator with having children (Table 3, Column 1), and find that childless workers who remain matched with the entrepreneur who brought them to the sessions sort 18 grams, or 5.2 percent, more than those who did not remain matched (p-value 0.075). This treatment effect is larger than that among workers with children (p-value of difference 0.069), which is itself not significant. This suggests that workers without children respond to social incentives, while those with children do not.

These results become slightly more pronounced if we only look at the sub-sample of workers who reported actual social ties to the entrepreneur with whom they arrived at the sessions. As shown in Column 2 of Table 3, among these workers, those without children sort 5.8 percent more beans if remaining matched with the entrepreneur who brought them to the sessions (p-value 0.075). The difference between this effect and that among workers with children (as captured by the interaction term between the treatment and having children) is now statistically significant at the five percent level (p-value 0.027).

The results of this heterogeneity analysis are consistent with the marginal utility of income being higher for parents, for example, due to the need to purchase food, clothes, or school materials for their children. In these circumstances, a higher marginal valuation of income may reduce the sensitivity of the effort response to variations in either the private or the social payoffs to additional effort. The model presented above formalizes this intuition; higher marginal private valuations of income p reduce the sensitivity of workers' optimal output to both variations in social incentives $a(s)$ and private incentives p for a sufficiently convex effort cost curve.

Our findings are also consistent with the possibility that parents may simply have less scope to increase their effort relative to non-parents. Suggestive evidence supporting this possibility is that in the control condition workers with children produce 4.9 percent more output, on average, than childless workers within the group of workers who report actual social connections to their employers (p-value 0.051, see Table 3, Column 2). While this may be due to differences in other characteristics between workers with and without children, it is consistent with parents exerting greater effort on average as their marginal valuation of earnings is higher. Put differently, parents may be unable to revise their effort upward as they already operate in a steeper section of their effort cost curve.

Table 3
Heterogeneity by Parental Status.

Variables	(1) Output	(2) Output Act.Soc.Tie
Remained Matched	18.014*	19.753*
	(10.095)	(11.032)
Children × Rem. Match	−21.295*	−28.428**
	(11.680)	(12.728)
Children	11.199	16.772*
	(6.825)	(8.518)
Female	1.603	−4.150
	(5.632)	(6.038)
Observations	313	203
Output Round 1	Yes	Yes
Session FE	Yes	Yes
Mean omitted category	349	343.2

Notes: Column 1 replicates Column 2 from Table 2, but interacting the indicator variable "Remained Matched" for a worker being matched with a socially connected employer with an indicator variable for the worker having children. Column 2 replicates Column 1, but restricting the sample to workers who reported actual social ties (kinship, friendship) to the employer who brought them to the lab sessions. All regressions control for output of worker in Round 1, worker gender, and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

However, a potential limitation of the approach used here is that given our relatively small sample size, we could not run the causal random forest, which identified having children as a potentially relevant dimension of heterogeneity, on a separate test sample from the one used for the main analysis. Thus, the overfitting concerns related to multiple-hypotheses testing mentioned above may also apply to the subsequent analysis in which we interacted the treatment with parental status. A way to assuage these concerns is to test whether parental status also mediates treatment effects in a new dataset from a similar but separate experiment, which is what we present in the next section.

4.4. Variations in private incentives

Workers may have had muted reactions to the variation in social incentives induced by our experiment because they have muted reactions to variations in *any incentives* in the type of real-effort task we

Table 4
Variations in Private Piece Rates.

Variables	(1) Output	(2) Output
Piece rate (TZS)	2.582	9.151**
	(2.587)	(4.588)
Piece rate (TZS) × Children		−9.292*
		(5.562)
Children		56.297*
		(28.973)
Observations	100	100
Gender	Yes	Yes
Sample FE	Yes	Yes
Mean omitted categ.	307.8	273.5

Notes: The table shows the results from regressing workers' output in a real effort task (sorting beans by color) on the piece rate that the workers received, with a randomly selected half of the workers receiving a per gram piece rate of 2 TZS, and the other half receiving 6 TZS. Column 2 interacts the piece rate variable with an indicator variable for whether the worker has children. The regressions control for worker gender. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

implemented. This may include muted reactions to variations in private monetary incentives (i.e., piece rates). To test for this possibility, we ran a follow-up experiment with 100 entrepreneurs in the same setting, 60 of whom had already participated in the original experiment, and 40 who were newly sampled.[13] Each of these entrepreneurs nominated again a worker, who did the same real-effort task as in the main experiment. However, this time we varied the piece rate paid to the worker, while we kept the piece rate paid to the entrepreneur constant.[14] We calibrated the variation in piece rates accruing to the worker to be consistent with the variation in the original experiment. Specifically, in this follow-up experiment workers were randomized into receiving either 2 or 6 TZS per gram of sorted beans, which corresponded to the worker's piece rate and the combined worker-and-employer piece rate, respectively, in the original experiment. In addition, workers received a fixed payment of 800 TZS, as in the original experiment, but adjusted for inflation.

Table 4 shows the results from the follow-up experiment. Consistent with our main results, we do not detect a change in effort when we increase private incentives in the overall sample (Column 1), but we find again a significant effect among childless workers (Column 2). This suggests that the effort of childless workers is sensitive to the strengthening of different types of incentives (i.e. both private incentives, in the form of higher personal piece rates, or social incentives, in the form of employer payoffs accruing to socially connected instead of non-connected employers). As mentioned above, workers with children, on the other hand, may have less room to respond to any incentives as they are already working harder to cater to their children's needs.

The heterogeneity analysis in the new experiment does not suffer from the potential issue of overfitting discussed in the previous session, yielding p-values of the correct size, because we are now using a separate sample from the one on which we estimated the Causal Random Forest. Finding consistent patterns across the two experiments is reassuring.

Note that the coefficient in Table 4 shows the effect per TZS of difference in piece rates. Given a difference in the piece rate between the two experimental groups of 4 TSZ, the difference in average output among childless workers between the groups is thus 36.6 grams. This is about twice as large as the difference induced by the equivalent variation in social incentives among childless workers in the main experiment (Table 3).

[13] We thank an anonymous referee for inspiring this follow-up experiment.

[14] For simplicity, in this follow-up experiment the entrepreneur did not receive any payments.

4.5. Heterogeneous effects: Kinship vs friendship ties

Finally, social ties can take very different forms, and in our baseline surveys, we collected information from both workers and entrepreneurs about the nature of the relationship they share with the person with whom they arrived at sessions. As already mentioned above, 65 percent of both workers and employers reported social ties to the counterpart, as opposed to simply referring to the other as "my worker" or "my boss". Out of the 65 percent, 41 percent of workers reported kinship ties to the entrepreneur who brought them to the sessions, and 59 percent friendship ties.[15]

Studying heterogeneous effects by type of social tie (friendship, kinship) is not only interesting in its own right but also as a means to conduct further tests of mechanisms through which social ties affect effort. For example, it could offer an alternative test for the presence of the retaliation channel, under the assumption that the retaliation channel may be more effective among friends than among kin. Reasons for that could be that there may be a larger number of levers for retaliation between friends than between kin (i.e. friendship can be ended, while kinship cannot), or given that there may be stronger (or more effective) push-back against retaliation measures within kinship networks relative to friendship networks (if the friendship in question is embedded in any friendship network at all).[16] Seen through the lens of our model, in particular its extension in Appendix C to include the retaliation channel, in kinship networks, the cost of retaliation c may be larger, while the effectiveness v of retaliation may be more limited.

Table 5 shows the effects of our experiment within the subsets of workers who report kinship ties (Column 1) or friendship ties (Column 2) to the entrepreneur who brought them to the session. As in the overall sample, we do not detect significant average effects in these two sub-samples (and the result is unchanged if we lump together both sub-samples, as was done in Column 3 of Table 2). In Columns 3 and 4 of Table 5, we replicate the heterogeneity analysis by whether workers have children on these two sub-samples, given that we did find significant treatment effects among childless workers. Interestingly, we find the effect to be large and statistically significant at the five percent level among childless workers with kinship ties to their employer, while the coefficient is insignificant and smaller among those connected by friendship ties (though the difference between the coefficients in the kinship and friendship sample is not statistically significant). Thus, the evidence does not support the retaliation channel, consistent with the test for retaliation shown in Section 4.2, which was based on the anonymous-worker variation of our experiment (see Table 2, Columns 4–5).[17]

[15] Eight further workers answered "neighbor" as type of relation. We decided to not count them as socially connected. As expected, the overlap in the nature of ties as reported by workers and entrepreneurs from the same pair was not perfect, with 17 percent of workers and 18 percent of entrepreneurs reporting no social ties to the other, while the other did report a social tie. In 54 percent of the pairs, both sides report the same type of tie (kin, friend, neither). Discrepancies may arise for various reasons. For example, workers may be both friends and employees of their employers, and different dimensions of the relationship may be more salient to the worker and the employer at the time of the experiment. Given that our core outcome of interest is the effort provided by workers, in case of discrepancies, we generally follow workers' responses about the nature of the relationship with the employer.

[16] We thank an anonymous referee for this suggested additional test for the retaliation channel.

[17] A further test for the retaliation channel driving the positive effect of social ties among childless workers consists of interacting the anonymous worker condition with an indicator for childless workers. However, as shown in Table B.4, the effect for childless workers is marginally significantly larger if their identity is hidden, opposite to what the retaliation channel would predict. This is additional evidence against the presence of the retaliation channel in our setting.

Table 5
Heterogeneity by Kinship vs Friendship Ties.

Variables	(1) Output Kin	(2) Output Friendship	(3) Output Kin	(4) Output Friendship
Remained Matched	−1.315	−4.862	43.244**	19.249
	(9.594)	(6.918)	(17.439)	(13.645)
Children × Rem. Match			−59.018***	−31.920*
			(20.779)	(16.763)
Children			20.591*	16.770
			(12.203)	(13.081)
Observations	75	128	75	128
Output Round 1 & Female	Yes	Yes	Yes	Yes
Session FE	Yes	Yes	Yes	Yes
Mean omitted category	365.3	354.4	339.2	346.2

Notes: Column 1 replicates Column 2 from Table 2 on the sub-sample of workers who reported kinship ties to the entrepreneur who brought them to the sessions, while Column 2 replicates it on the sub-sample of workers who reported friendship (but no kinship) ties to the entrepreneur. Columns 3 and 4 replicate instead Column 1 of Table 3 on these two sub-samples of kinship and friendship connected workers. All regressions control for output of worker in Round 1, worker gender, and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

4.6. Further mechanisms and results

We conclude the presentation of our results with a number of further tests of additional explanations and mechanisms.

Signalling of type by re-matched workers

A hypothetical reason for not detecting a significant effect in the overall sample may be that while workers who remained matched with the entrepreneur they arrived with increased their output due to altruism, those who were re-matched increased effort too, but for a different reason: *signalling their type* (in terms of ability or willingness to exert effort) to the unknown entrepreneur to whom they were re-matched. We expect ex-ante this signalling effect to be small, if present at all, because workers and entrepreneurs were located in different rooms and thus had limited opportunities to meet (let alone discuss employment opportunities in real life). Furthermore, given the stylized nature of our real-effort task, ability signalled in the experiment may have limited information-value for the tasks conducted in real-world firms.

Nevertheless, one tentative test for this mechanism is to study whether re-matched workers exert less effort when their identity is concealed in the anonymous worker variation arm. As shown in Table B.6, that is not the case. If anything, re-matched workers exert more effort in the anonymous worker variation of the experiment, though the effect is not significant.[18]

Heterogeneity by entrepreneur–worker income difference

Another explanation for the insignificant overall effect of our treatment could be that the average worker does not sufficiently internalize the utility of the connected employer because employers are considered relatively rich. Thus, the marginal utility of added pay-offs to them may be (perceived) too small to significantly affect the utility of the worker. In our baseline survey, entrepreneurs report, on average, 70 percent higher income than workers (190 vs 113 US$ monthly).

In Appendix Table B.7, we replicate the core regression from Table 2, Column 2, but interacting the "Remained Matched" indicator with a dummy for the income difference between the worker and the entrepreneur with whom the worker arrived at the session to be above the median. If the lack of an average treatment effect was due to large income differences between workers and entrepreneurs, we would expect a negative coefficient on this interaction. We do not find evidence supporting this hypothesis.[19]

Consistency of worker effort provision

Socially connected workers may not on average produce more than workers without social ties to their employers, but may have less variability in their output. This may explain why, in real life, risk-averse employers may still prefer to hire socially connected workers. We test this hypothesis by first obtaining the residuals from a regression of output on session fixed effects and a dummy equal to one if the worker is connected to the employer. We then regress the *absolute value* of these residuals, as a measure of the deviation in the output of the worker from the level that could be expected from him or her on average, on session fixed effects, and the usual "Remained Matched" dummy. As shown in Table B.8, there is neither a difference in these deviation measures between connected and non-connected workers in the overall sample (Column 1), nor in the sub-samples of workers reporting actual social ties (e.g., friendship, kinship) to the entrepreneur who brought them to the sessions.[20]

5. Conclusions

An active literature is interested in whether workers hired via social networks perform better. These studies usually find that such workers

[18] This result has to be interpreted with caution, as the anonymous worker variation was not randomly allocated to sessions. In the main analysis of the experiment, this is addressed through inclusion of session fixed effects. However, these fixed effects cannot be included in Table B.6, as they would prevent the estimation of the key coefficient on the Anonymous Worker session indicator.

[19] The extent to which workers in our experiment internalize entrepreneurs' utility also depends on which workers among those in their networks the entrepreneurs selected for the sessions. Fig. 1 suggested that workers are selected based on skill and trust, but not based on the strength of their social ties to the entrepreneur. A sub-sample that may overall be less affected by selection issues is the one of workers brought by entrepreneurs who employ exactly one person in their firm (and, therefore, had limited choice over which worker to bring). As shown in Table B.2, there is a marginally significantly stronger effect of remaining matched for these workers (Column 5), which, however, is not significant to restricting the sample to workers who report actual social ties to their employer (Column 6).

[20] Note that the variation in output we are investigating here is variation *conditional on the type* of the worker, which is distinct from variation in the type of the worker. The latter may be minimized by careful screening at the hiring stage and may also be eased by hiring through social networks (Dustmann et al., 2016), but is not the focus of this paper. As discussed above, we focus on effort choice conditional on type, and we keep the type distribution constant between the remained matched and re-matched groups due to the random allocation of workers to each group. Output variation conditional on type then captures the consistency in effort choice by (connected or unconnected) workers.

are better selected, but it remains unclear whether these workers also exert different levels of effort at work, conditional on their type. We address this question using a novel experiment conducted with pairs of real-world "workers" and "entrepreneurs" in Tanzania. Our design allows us to exogenously "break" social ties and isolate cleanly the effect of social connections on the effort choice of workers.

We find that workers who work for an employer they know exert higher effort if they do not have children. Over the full sample, we do not find evidence that being connected to one's employer increases workers' effort. This is despite being powered to detect differences of as little as 3.3 percent of output between socially connected and unconnected workers. In terms of mechanisms, several distinct tests point against the hypothesis that fear of retaliation from the employer or expectations of different rewards drives the behavior of socially connected workers, in particular the positive effort response from childless workers. This suggests that the effects we find among childless workers are driven by the altruism of workers towards employers with whom they are connected. Our results are consistent with a simple model of effort choice in which having children increases the marginal valuation of private income and crowds out altruistic motives towards one's employer. If parents have a higher private valuation of income, they might exert higher effort absent the treatment and thus have limited scope to react to social incentives.

The muted role of the retaliation channel could be due to the fact that retaliation by socially connected employers may be difficult if other obligations are embedded in the social networks that connect them. A rich literature has explored the importance, multidimensionality, and constraining effects of social networks as a basic insurance mechanism in Tanzania (De Weerdt and Dercon, 2006; De Weerdt and Fafchamps, 2011) and in other low-income settings (Fafchamps and Gubert, 2007b,a; Munshi and Rosenzweig, 2016). Hiring workers from social or kinship networks may entail other valuable rewards for an entrepreneur, such as support by the worker (or his/her family) in case of negative economic shocks to the entrepreneur. These parallel concerns may limit the ability of the entrepreneur to punish low effort in the game. We plan to follow up on these questions in future research.

Finally, although our interest in this paper is in workers' effort, our design could be used to study the effects of social ties on many other aspects of employment relations, such as remuneration or investment decisions by employers, or bargaining between employers and workers. The design could also be used to study the effects of social ties between workers, and not just between workers and employers. For that purpose, one would invite pairs of workers and rematch a randomly selected subset of workers, to study, e.g., contributions to joint work projects or the allocation of time to individually vs jointly beneficial tasks.

CRediT authorship contribution statement

Martin J. Chegere: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Paolo Falco:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Andreas Menzel:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Different rewards for socially connected workers

In this section, we report results from two supplementary variations of the basic experimental design, each implemented in a set of eight out of the 32 sessions. These variations were implemented to test whether employers reward workers to whom they are socially connected differently, conditional on the output they produce. The first variation tests if they are rewarded differently ex-post, i.e. after they produced the output, while the second one tests whether they are rewarded differently ex-ante, i.e. promised a different piece rate before they start producing. As shown in more detail below, we find no evidence of either reward pattern.

Different rewards ex-post: Entrepreneurs can tip

In the first variation, matched entrepreneurs and workers went through the same experimental protocol as in the basic version of the game, except that entrepreneurs had the opportunity to "tip" their matched worker after the worker's output was revealed. For that, the entrepreneur received an extra 1500 TZS at the end of the session, out of which he or she could give any amount to the worker. Importantly, workers in these sessions were informed about the possibility of being tipped before they started the real-effort task.

In Table A.1, Column 1, we study whether connected vs unconnected entrepreneurs set different tip levels *unconditionally* (i.e., without controlling for workers' effort). We find that entrepreneurs give seven percent *lower* tips to connected workers, a difference which is not statistically significant. This result does not change when controlling for the level and squared level of the output of the worker (Column 2), which is not surprising, given that the output levels do not differ, on average, between connected and unconnected workers. These results do not change if we restrict the sample to workers who reported social ties to the entrepreneur who brought them (Column 3). Finally, the effect of remaining matched on workers' output is not different in the sessions in which entrepreneurs had the opportunity to tip, neither in the whole sample (Column 4) nor within the sample of workers reporting actual social ties to the entrepreneur (Column 5).

Different rewards ex-ante: Entrepreneurs set the piece rate

In this variation, the game is played as in the baseline version with employers and workers knowing each other's identity. However, the piece rate for the worker is not pre-determined; instead, the combined piece rate for the worker and the entrepreneur of 6 TZS is offered to the entrepreneur, who has to decide how to split it between the worker and him- or herself. That is, the entrepreneur determines the piece rate of the worker and can decide to pay a different rate to workers to whom he or she is connected. The piece rate is announced to the worker before the start of the work.

Different piece rates for connected vs unconnected workers may arise out of purely self-interested reasons for the entrepreneur. For example, socially connected workers may have a higher intrinsic motivation to put effort into the work, which could allow the entrepreneur to set a lower piece rate, keeping more of the revenue received from the output for themselves. Alternatively, the effort of connected workers may be more elastic to the piece rate. In that case, setting a higher piece rate for a connected worker may be optimal. Finally, connected workers may receive a higher piece rate purely out of altruism by the entrepreneur. We are not able to distinguish altruism from self-interested reasons, and we focus on testing whether rates differ at all.

As shown in Column 1 of Table A.2, entrepreneurs offer a statistically insignificantly 10 percent higher piece rate if they remain matched with their connected workers. This fits with our baseline

Table A.1
Opportunity to Tip for Entrepreneurs.

Variables	(1) Tip	(2) Tip	(3) Tip Act.Soc.Tie	(4) Output	(5) Output Act.Soc.Tie
Remained Matched	−41.877	−32.494	−28.316	5.555	−1.515
	(55.891)	(56.617)	(67.834)	(5.051)	(6.446)
Output Round 2		3.118	3.346		
		(2.753)	(3.690)		
Output Round 2^2		−0.003	−0.003		
		(0.004)	(0.005)		
Remained Matched × Tips				−13.500	−0.255
				(8.689)	(11.322)
Observations	81	81	53	313	203
Output Round 1	–	–	–	Yes	Yes
Session FE & Female	Yes	Yes	Yes	Yes	Yes
Mean omitted category	623.9	623.9	604.3	373.5	373.2

Notes: The first column tests whether entrepreneurs who were randomly selected to remain matched with the worker they brought to the session chose a different tip level (after the output of the worker was revealed) than entrepreneurs re-matched with other workers from the same session. The sample is from eight sessions (out of 32) in which entrepreneurs could give tips. Column 2 controls for the level and square of the output produced by the matched worker. Column 3 replicates Column 2 on the sample of workers who reported social ties (e.g., friendship, kinship) to the entrepreneur who brought them to the sessions. Column 4 tests if the effect of being matched with a connected entrepreneur on workers' output is different in the sessions where entrepreneurs could tip, and Column 5 does so again among the workers reporting actual social ties. All the specifications control for worker gender and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

Table A.2
Entrepreneurs Set Piece Rates.

Variables	(1) Rate	(2) Rate Act.Soc.Tie	(3) Output	(4) Output	(5) Output Act.Soc.Tie
Remained Matched	0.204	0.208	2.182	2.132	−1.758
	(0.223)	(0.316)	(5.000)	(5.019)	(6.802)
Remained Matched × Endog. Rate			−0.768	0.979	1.124
			(8.627)	(8.366)	(9.804)
Rate				−15.493	−0.276
				(12.395)	(9.301)
$Rate^2$				1.591	−0.829
				(2.487)	(2.059)
Observations	76	50	313	313	203
Output Round 1	–	–	Yes	Yes	Yes
Session FE & Female	Yes	Yes	Yes	Yes	Yes
Mean omitted category	1.938	2	356.9	356.9	361.1

Notes: The first two columns test whether entrepreneurs who were randomly selected to remain matched with the worker they brought to the session choose a different piece rate to be paid to the worker for every gram of correctly sorted beans compared to entrepreneurs re-matched with workers unknown to them. The sample is from eight sessions (out of 32) in which entrepreneurs set the piece rate. Column 2 replicates this test for workers who reported actual social ties (e.g., friendship, kinship) to the entrepreneur who brought them to the session. Column 3 tests whether the effect of working for a socially connected entrepreneur on workers' output is different when the entrepreneur can set the piece rate. Column 4 replicates the previous column, but controls for the endogenous piece rate set by the entrepreneur (by inserting both a linear and a quadratic term). Finally, Column 5 replicates Column 4 on the sample of workers actually connected to the entrepreneurs who brought them. All columns control for worker gender and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

survey, in which only nine percent of entrepreneurs stated they would set a lower piece rate for socially connected workers, while six percent would set a higher rate, with the remaining 85 percent not adjusting the rate they would offer. Entrepreneurs matched with random workers set on average a piece rate of 1.938 TZS, very close to the exogenously set rate of 2 TZS in the sessions in which the entrepreneurs do not choose the piece rate. The difference in piece rates remains unchanged in the sample of actually connected workers (Column 2). The ability of entrepreneurs to set piece rates for workers does not change the effect of social ties on workers' effort (Column 3). We also do not detect a correlation between the endogenously set piece rate and worker output (Column 4), and these results are not different in the subsample of workers who reported actual social ties to the entrepreneur with whom they arrived at the sessions (Column 5).

Appendix B. Additional tables

See Tables B.1–B.8.

Appendix C. Extensions of conceptual framework

This Appendix discusses extending the model from Section 3 in two directions, to incorporate the retaliation mechanism, as well as entrepreneurs potentially caring about workers' effort cost.

C.1. Incorporating retaliation channel

We first extend the model to allow employers to punish workers for too low output, and show that with complete information (i.e. by the entrepreneur about the cost of effort for the worker), the retaliation channel implies a positive effect of social ties on workers' output, under mild modelling assumptions. The predictions are less clear-cut for models with imperfect information.

We incorporate retaliation by assuming that the employer can pay a cost c which allows him to reduce the utility of the worker by v by exerting a punishment (Becker, 1968). Assume that the employer's utility function is $U_E = e - 1[e < \mu]c$, where e is the output produced by

Table B.1
Difference in Output between Round 1 and 2 as Outcome.

Variables	(1) Diff. Output	(2) Diff. Output	(3) Diff. Output Act.Soc.Tie	(4) Diff. Output	(5) Diff. Output Act.Soc.Tie
Remained Matched	3.974	1.997	−1.581	−0.394	−0.633
	(4.276)	(4.181)	(5.386)	(4.366)	(5.549)
Rem. Match × Anonymous Worker				9.852	−3.534
				(11.583)	(13.979)
Observations	313	313	203	313	203
Output Round 1 & Gender	–	Yes	Yes	Yes	Yes
Session FE	Yes	Yes	Yes	Yes	Yes
Mean omitted categ.	19.31	19.31	20.85	20.85	20.85

Notes: The table replicates Table 2, but with difference in workers' output between Round 1 and 2 as the outcome variable. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

Table B.2
Heterogeneity of Treatment Effects by Worker Status, Tenure, and Firm Size.

Variables	(1) Output	(2) Output Act.Soc.Tie	(3) Output	(4) Output Act.Soc.Tie	(5) Output	(6) Output Act.Soc.Tie
Remained Matched	2.489	−2.872	0.870	−4.643	−5.218	−5.500
	(8.033)	(8.909)	(4.740)	(6.063)	(5.506)	(7.055)
Actual Worker × Rem. Match	−0.752	2.210				
	(10.060)	(11.464)				
Actual Worker	7.025	4.760				
	(6.237)	(7.703)				
Months known × Rem. Match			0.030	0.080		
			(0.049)	(0.060)		
Months known			−0.015	−0.049		
			(0.034)	(0.046)		
One worker × Rem. Match					19.690*	10.884
					(10.029)	(11.421)
One worker					−5.387	−3.155
					(5.980)	(7.384)
Observations	311	201	311	201	313	203
Output Round 1 & Gender	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean omitted categ.	357	357.5	356.4	358.4	355.6	359.1

Notes: Columns 1–2 interact the treatment indicator "Remained Matched" with an indicator variable for whether the worker was actually employed by the employer who brought him/her to the sessions, as opposed to being a potential employee if need arose. Columns 3–4 show equivalent interactions with number of months the worker reported to know the employer who brought him to the sessions. Columns 5–6 show equivalent interactions with an indicator for the employer employing a single worker in his/her business. "Act.Soc.Tie" in column header indicates that sample is restricted to those workers who reported to share actual social ties (friendship or kinship) to the employer who brought them to the sessions. All regressions control for the gender and the output of the worker in Round 1, and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

Table B.3
Gender Heterogeneity in Overall Treatment Effects.

Variables	(1) Output	(2) Output Act.Soc.Tie
Remained Matched	1.904	−0.462
	(7.112)	(8.795)
Remained Matched × Female	0.155	−1.982
	(9.022)	(10.613)
Female	3.083	−1.035
	(5.993)	(6.959)
Observations	313	203
Output Round 1	Yes	Yes
Session FE	Yes	Yes
Mean omitted category	332.6	337.5

Notes: Column 1 replicates Columns 2–3 of Table 2, but interacting the treatment indicator variable "Remained Matched" with an indicator variable for the worker being female. All regressions control for worker output in Round 1, and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

the worker (equal to his effort), and $1[e < \mu]$ is a dummy indicating whether the employer punished the worker or not. Assume μ is a threshold value for output that the employer chooses and announces to the worker, with punishment occurring for output lower than μ.[21] If the optimal output of the worker, as determined in Eq. (2) in Section 3, is below the punishment threshold ($\mu > e^*$), the worker will increase effort from e^* to μ if $d_U(\mu, e^*) = (e^* - \mu)(p + a(s)) + \mu^\gamma - e^{*\gamma} \leq v$, i.e., if the reduction in utility for the worker from producing μ instead of e^* is smaller than the loss of utility v from punishment. On the other hand, if $\mu > e^*$ and $d_U(\mu, e^*) > v$, the worker will not adjust output and receive the punishment. Finally, if $\mu < e^*$ the punishment is not binding and does not affect workers output.

We next derive the optimal punishment threshold μ from the perspective of the employer. Note, first, that for $\mu \geq e^*$, $\delta d_U(\mu, e^*)/\delta\mu > 0$. Therefore, there exists a threshold level μ' at which $d_U(\mu', e^*) = v$, with $d_U(\mu, e^*) < v$ for $\mu < \mu'$, and $d_U(\mu, e^*) > v$ for $\mu > \mu'$. For values of $\mu \leq e^*$, employers do not gain utility through higher output, nor lose utility by having to exert punishment. For $e^* < \mu \leq \mu'$, the worker will increase output, leading to an increase in utility for the employer, and again no loss from punishment having to be triggered. Finally, for $\mu > \mu'$ the worker will not adjust output, and the employer will lose utility from punishing the worker. The employer's utility $U_E = e - 1[e < \mu]c$ is thus maximized at the largest μ at which the worker still produces $e = \mu$, which is $\mu = \mu'$.

[21] For this basic exploration of the retaliation channel, we abstract away from potential inabilities of employers to commit to punishments.

Table B.4
Anonymous Worker Condition among Childless Workers.

Variables	(1) Output	(2) Output Act.Soc.Tie
Remained Matched	2.255	12.772
	(8.981)	(12.090)
Rem. Match × Anonymous Worker	54.722*	21.479
	(29.371)	(26.430)
Rem. Match × Childr. × Anon. Worker	−64.070*	−36.031
	(33.427)	(31.532)
Children × Anonymous Worker	30.584*	16.566
	(18.139)	(22.623)
Children × Rem. Match	−3.282	−17.609
	(10.554)	(13.522)
Children	1.793	11.993
	(6.893)	(8.879)
Observations	313	203
Output Round 1 & Female	Yes	Yes
Session FE	Yes	Yes
Mean omitted category	360.1	343.5

Notes: Column 1 replicates Column 1 of Table 3, but interacting the treatment indicator "Remained Matched", and the indicator for the workers having children, with an indicator variable for the worker participating in one of the sessions in which the game was played in the "anonymous worker" condition, i.e. the identity of the worker was withheld from their matched entrepreneur. Column 2 restricts the sample to workers who report actual social ties (kinship, friendship) to the entrepreneur who brought them to the sessions. All regressions control for worker gender and output in Round 1, and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

Table B.5
Causal Random Forest Results.

Variable	p-value for prediction of CATEs
Children	0.054
Muslim/Christian	0.087
Literate	0.125
Earning cash	0.131
Household size	0.138
Average	0.413

Notes: The table shows the p-values from relating observation-level predicted treatment effects (CATEs) obtained from the Causal Random Forest (CRF) to 18 covariates individually, using best linear projection. The table shows the five variables with the lowest p-values, and also the average p-value across all 18 co-variates. The 18 variables are: Worker age, Years schooling, Gender, Muslim/Christian, Married, Having children, Household head, Household size, Literate, Being employed, Earning cash, Being employed for employer who brought worker to session ("Actual Worker"), Total income, Pay taxes, Social ties to employer who brought worker to session ("Actual Social Ties"), Nbr employees of employer who brought worker to session, Years living in Dar Es Salam, Output Round 1.

Table B.6
Signalling by Re-Matched Workers.

Variables	(1) Output	(2) Output Act.Soc.Tie
Remained Matched	−1.154	0.120
	(4.511)	(5.287)
Rem. Match × Anonymous Worker	10.209	−1.917
	(11.710)	(13.471)
Anonymous Worker	1.194	9.638
	(7.636)	(10.638)
Observations	313	203
Output Round 1 & Female	Yes	Yes
Session FE	No	No
Mean omitted category	360	357.9

Notes: Column 1 replicates Column 4 of Table 2, except for not including session fixed effects, which allows for the estimation of the "Anonymous Worker" coefficient (which is otherwise co-linear with session fixed effects). Column 2 restricts the sample to workers who reported actual social ties (kinship, friendship) to the entrepreneur who brought them to the session. All the specifications control for worker gender and output in Round 1. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

Table B.7
Heterogeneity by Entrepreneur–Worker Income Difference.

Variables	(1) Output	(2) Output Act.Soc.Tie
Remained Matched	0.853	−1.961
	(5.768)	(7.057)
Remained Matched × High income diff.	2.468	1.165
	(8.647)	(11.460)
High income difference	−4.432	−4.403
	(6.321)	(8.015)
Observations	313	203
Output Round 1 & Female	Yes	Yes
Session FE	Yes	Yes
Mean omitted category	356.6	357.7

Notes: The table replicates Columns 2–3 from Table 2, but interacting the key treatment indicator "Remained Matched" with an indicator for the income difference between the worker and the entrepreneur with whom the worker arrived at the session being above the median difference in the sample. All regressions control for worker gender and output in Round 1, and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

Table B.8
Deviation of Output from the Average.

Variables	(1) Outp. Deviation	(2) Outp. Deviation Act.Soc.Tie
Remained Matched	−0.368	−3.723
	(3.811)	(4.726)
Observations	313	203
Output Round 1 & Gender	Yes	Yes
Session FE	Yes	Yes
Mean rematched worker	45.21	45.07

Notes: The table replicates Columns 2 and 3 of Table 2, but using as outcome variable the absolute distance of a worker's output from the average output of workers in the same session and with the same treatment (remained matched, re-matched). All regressions control for worker gender and output in Round 1, and session fixed effects. Robust standard errors in parentheses: * $p < .1$; ** $p < .05$; *** $p < .01$.

How would the effectiveness of the retaliation channel, i.e. the value of μ', be affected by the presence of social ties in our model? We make *one* assumption, which is that social ties (weakly) increase v, the amount of worker's utility loss an employer *can* impose if retaliating. That is because social ties provide the employer with additional levers to punish the worker. On top of firing the worker or reducing pay, an employer can also, for example, ostracize a socially connected worker on other occasions in which they meet, such as family or friendship gatherings, or tell other people about the low effort. Note that we do not assume anything about how social ties affect the cost c of punishment, which may decrease or increase if employers have social ties to workers. For example, they may increase if the employer receives push-back from other people in the shared social network for punishment, or may lose a friendship if s/he punishes a worker connected to them within a larger friendship network. But as we show, in equilibrium, at least with full information about workers' effort costs (and thus μ'), the employer will never have to bear the punishment costs c, and thus any effect of social ties on costs c does not affect the optimal punishment threshold μ'. But if social ties increase v, they also increase μ', given that $d_U(\mu', e^*) = v$, and given that for $\mu \geq e^*$, $\delta d_U(\mu, e^*)/\delta\mu > 0$.[22]

[22] Imperfect knowledge would affect this result if it implies that employers do not perfectly know the threshold values μ' of the workers, i.e. they only have a prior belief CDF $F(\mu')$ over those values. In this case, each potential threshold level μ set by the employer comes with a probability $F(\mu)$ that

C.2. Employer caring about worker's effort costs

The model can also be extended to allow for the employer caring about the effort costs of socially connected workers. We do so by assuming again a very simple utility function for the employer $U_E = e - a_E(s)e^{\gamma}$, where e is the effort of the *worker*, which we assume to be equal to the output, and e^{γ} again the worker's effort costs, while $a_E(s), \quad \delta a_E(s)/\delta s \geq 0$ is the weight the employer puts on the effort cost of the worker, which depends on the strength of social ties s. If we then assume again that the worker internalizes the employer's utility, we get an updated utility function U_W of the worker, $U_W = (p+\alpha(s))e-(1+\alpha(s)a_E(s))e^{\gamma}$. The new first order condition to determine optimal effort is then:

$$e^* = \left(\frac{p+\alpha(s)}{\gamma(1+\alpha(s)a_E(s))}\right)^{\frac{1}{\gamma-1}} \tag{4}$$

The fact that employers care about workers' effort costs reduces the sensitivity of worker effort to the presence of social ties to the employer. In fact, the effect of increasing social ties $a(s)$ on e^* turns negative if employers' altruism becomes strong enough relative to workers' private incentives, or $a_E(s) > 1/p$. In that case, the altruism channel could operate in the opposite direction of the retaliation channel, potentially leading to an overall zero effect of social ties, while each channel on its own does have a non-zero effect.

Appendix D. Experimental scripts

D.1. Invitation script for entrepreneurs

We would like to invite you to take part in a research project to understand decision making among entrepreneurs like you. The study is being done by researchers from the University of Dar-es-Salaam, together with further researchers from abroad. If you like to participate, we invite you to come next week, on a day and time we can fix in a moment, to the University of Dar-es-Salaam, to a place we will describe you. You would participate in a session there that would last around 2–3 hours.

In this session, we will make an interview with you about how you run your business which will last around 30 minutes, and you will participate in some games designed to understand how entrepreneurs like you make decisions. The games are very simple, we will explain them to you at the start of the session there. You will receive 3000 TZS just for participating in the study. But you will earn more money during the games. The exact amount of money will depend on how the games go in which you will participate, but most entrepreneurs will typically earn between 6000 and 9000 additional TZS in the games, on top of the 3000 TZS they get just for participating in the study. We will explain you everything again at the start of the session if you agree to come. If you do not like anything you hear or see when you come, you will be free to leave anytime again. Once more, the study is all about how you run your business, for example how you select workers that may work for you, or how you motivate them. Even if you currently have no workers, you can still participate, however.

To participate in the session, you will have to bring with you another person to the session, who will spend the same time there as you. That person will also be interviewed and also participate in the games. The person will also get 3000 TZS just for showing up, and typically also earns between 6000 and 9000 TZS during the games. It is your choice who to bring along to the session, and you do not have to tell us now. Just bring a person with you to the session. But it should be a person who you can see as working for you in your business, either full-time or part-time. The person need not be working for you at the moment, though it can be a current worker of yours. But if it is not a current worker of yours, you should at least think that if you need an additional help someday, this could be a person you could employ. In the games we play, you will take the role of a boss, while the person you bring along will take the role of a worker. Therefore, the person should have no problem to accept you as their boss in a work environment. There will be around 10 other entrepreneurs like you coming to the sessions that will also be interviewed and play these games.

Do you have any questions? DO YOU LIKE TO PARTICIPATE? [YES/NO]

D.2. Script for the experimental sessions

1. Greeting, Introduction, Consent (for ENTREPRENEURS and WORKERS)

Dear all, thank you very much for participating in this study today. What we are doing here today is for research on how businesses in our country are working and how people working in the same business interact. It is about making the business environment better in Tanzania, and in many similar countries, and thereby help all the people that work in these businesses. So welcome here once more.

There will be two parts of the sessions we want to go through with you today. First some short exercises or games as we call them. These games are there to mirror a work-day, and help us to understand how people think about their work. Then we will do an interview about you and your work. All in all, the session here today will take about 2–3 hours.

Let me emphasize that you do not have to participate in this session if you do not like to. You can leave this room anytime. There is no obligation for you to start this session, or finish it. After I explain what we want to do today, I will ask you once more if you want to take part in the study, or not. You can leave by that time, or even any time after that.

You will for sure get 3000 TZS for having showed up here. You will get this money also if you decide to leave before the end of the session today. But during the games we will play later on, you will have the opportunity to earn more money, depending on how you play the game, and also depending on luck. We expect that most of you will earn between 6000 and 9000 additional TZS from finishing the whole session today. Please be aware that not all of you will earn exactly the same amount of money here today. Think of it a little bit like a lottery, some of you will be a bit luckier than others and these differences are necessary for out study. But rest assured that each of you will get at least 3000 TZS for just having come here today.

[For ENTREPRENEURS: You have been personally invited by a staff member of the University of Dar es Salaam to come to this session today, and have been told that you should bring a person along who you can see working for your business if you need help. Today you will, therefore, play the role of "BOSS". The person you have come with is now in another room and will play the role of a "WORKER". Let me tell you now that all of you who are in this room will be BOSSES in this session. "BOSSES" and "WORKERS" will play a different role in the games, and go through a different interview with different questions after the games].

the distance $\mu - e^*$ is too large for the worker to adjust effort, and thus punishment will have to be exerted, resulting in a loss of payoff for the employer. Intuitively, then, if social ties increase both v and c, then social ties would on the one hand allow employers to set a higher punishment threshold, as a larger v would induce the worker to deviate more from the optimal effort level e^*. On the other hand, a higher c would make punishment more costly in case it has to be exerted, which would, ceteris paribus, induce the employer to set a lower punishment threshold, to reduce the probability that punishment has to be exerted. The net effect on the threshold level thus depends on the relative effect of social ties on v and c. In that case, the presence of social ties will not unambiguously strengthen the retaliation channel, if we allow social ties to increase the cost of punishment c.

[For WORKERS: You have been invited to the session today by an acquaintance, maybe your chief at work, or a relative or a friend who was personally invited by a staff member of the University of Dar es Salaam to come to this session today. In that case, therefore, today you will play the role of a "WORKER". The person who invited you is now in another room and will play the role of a "BOSS". Let me tell you now that all of you who are in this room will be WORKERS in this session. "BOSSES" and "WORKERS" will play a different role in the games, and go through a different interview with different questions after the games].

In the next hour or so, we will play the game sessions. Before the start of the games, we ask you to sign a form that you agree to participate in this session today. If you do not agree, you are free to leave the session for good. Remember though that even after signing the form, if you are uncomfortable with anything, you can quit the session and leave. But we want to assure you that we do not want to ask any indecent question, and also the game is easy and simple. After we have finished the game session, staff members will conduct an interview with you, asking you about your work, and a bit of background information about you. We will not be able to interview all of you at the same time, so we will just start with some of you and ask the others to wait until it is their turn.

Does anyone have any questions? … LET EVERYONE SIGN THE CONSENT FORM

D.2.1. Script for WORKERS

Now we start with the first part of the session today, in which we simulate a workday through a simple exercise. Overall, this session will take around 1.5 hours in total. After that, those of you who have already been interviewed will be free to go, the rest should stay for a short questionnaire.

As we said earlier, in the game you will play the role of a WORKER. As a WORKER in the game, you will carry out a simple task. Starting from a large bag with three different kinds of beans, we would like you to sort as much as possible of one type of bean in 8 minutes in these cups [show small transparent cups]. For playing the game, a fixed amount of 700 TZS will be paid, and in addition, for every gram of beans that is correctly sorted, you will receive some money. We will make it clear how much money you will get before each round of the game. But the harder you work, and the more amount of beans of one type you sort, the more money you can earn during the game sessions. As part of today's game, you will have a chance to carry out 2 or 3 rounds of this task.

Do you have any questions at this point? [ANSWER ANY QUESTIONS]

Let me now show you the place where you will play the game. You will have a sack of beans and empty plastic cups. Remember that you should fill the small cup with just one type of bean. A cup with one type of beans is considered well sorted if it contains no more than 6 grains of other types of beans. For playing the game, a fixed amount of 700 TZS will be paid, and in addition, for each gram of sorted beans, you will get 2 TZS in the first round of the game.

Before we start, please take 5 minutes to try out the task, familiarize yourself with the beans, the cups and the weight. This is NOT the first round, you will not get paid for the beans you sort in the next 5 minutes. This is just so that you can familiarize yourself with the task. The first round of the game, when you will get paid, starts later. Do not worry, I will clearly tell you when it will start.

[Give 5 minutes so that workers can try out the task. Answer any question they may have.] Now we can start

Round 1:

In the first round of the game, that we will start in a moment, you will simply do the task that I just explained to you. For each gram of sorted beans, you will get 2 TZS, in addition to the fixed payment of 700 TZS.

Are there any questions? [Answer any questions]

Let us start Round 1 now. You have 8 minutes to sort as much of the beans as you want. After the 8 minutes are over, a staff member from our team will come to your table, weigh the beans you have sorted and note the amount.

Round 2:

Now let us play the game a second time. In Round 2, you will have again 8 minutes to sort as many of the beans as you can, and you will get some money for each gram of beans you sort. But there is now an additional element in the game. For Round 2 you have been matched with one of the BOSSES that are here today, who will be your BOSS. Your boss will also get some money for each bag that you produce. You will get some money for each gram of beans that you sort, and a fixed payment of 700 TZS, as in Round 1, but now also your boss gets some money for each gram of beans sorted. So, the more beans you sort, the more money you will earn, as well as your boss will earn. But your boss will not help you with sorting the beans. He/She will not even watch you. He/She will stay in a different room. He/She will just get money depending on how much you produce. Think of it as if your boss gave you the job, and he/she sells the beans that you sort. He/She keeps some of the money from each gram of sorted beans he/she sells, and you get some of the money as payment for sorting them.

Recall that bosses are the business owners that the staff members of University of Dar-es-Salaam have invited to this session, and who had to invite one person they knew to come to the session today, just like you. Your boss in the game can either be the person who invited you to come today, or another of these persons invited by our staff.

Do you have any questions? Then let us see who your boss is for you for round 2.

[SHOW NAME OF THE BOSS]

a. Addendum: Round 2, Group A: Baseline Game

In this game you will be paid 2 TZS for each gram of beans you sort correctly, in addition to the fixed payment of TZS 700. The Boss matched with you will also get 4 TZS for each gram of beans you sort correctly.

Last important information before playing Round 2. Your boss whose name you just saw knows that you are the worker he/she is matched with in this round.

b. Addendum: Round 2, Group B: Boss does not know Worker

In this game you will be paid 2 TZS for each gram of beans you sort correctly, in addition to the fixed payment of TZS 700. The Boss matched with you will also get 4 TZS for each gram of beans you sort correctly.

A final important detail before we play Round 2. Your boss, whose name you just saw does not know that you are his/her worker! This is very important. While you know who your boss is, this person does not know that you are the person working for him/her. Why is that important? Because when your boss gets the money for each gram of beans that you sort, he/she does not know who sorted the beans. Thus, if he/she is disappointed, or, maybe, very happy about the money he/she got, he/she does not know who he/she can be angry with, or who he/she can thank. We do not tell him/her who was the worker sorting the beans for him/her. Only you know who was the worker that worked for him/her –you! So you do not need to worry about what your boss thinks about the amount of beans that you sorted in Round 2. You can freely decide for yourself how much beans you want to sort in the 8 minutes, and how much money you want to earn accordingly.

c. Addendum: Round 2, Group C: Boss Can "Tip"

In this game you will be paid 2 TZS for each gram of beans you sort correctly, in addition to the fixed payment of TZS 700. The Boss matched with you will also get 4 TZS for each gram of beans you sort correctly.

There is one more thing that your boss will do in this game. In addition to the money from the sorting of beans, your boss will get 1500 TZS extra. After the 8 minutes are over, and your boss learns how much money he/she will get based on the amount of beans you have sorted, he/she will be asked whether he/she wants to share some of the extra

1500 TZS with you, as an extra reward for you, or a tip. This is purely voluntary, and fully his/her decision. It will be a way of thanking you for doing a good job if he/she wants to. This would come on top of the money that you will get in any case from sorting the beans as in the previous round.

d. Addendum: Round 2, Group D: Boss sets Worker Rate

The important role of your boss in this second round is that he/she decides how many TZS you get for each gram of beans you sort. It works like this. For each gram of beans you sort, your boss gets a total of 6 TZS. But before the start of Round 2, he/she has to decide how many of the 6 TZS to give to you for each gram. For example, he/she could say that out of the 6 TZS, he/she gives you 2 TZS. Then he/she would get 4 TZS for each gram of beans sorted. In addition to the payment you receive per gram, you will also get an additional fixed payment of TZS 700. Your boss has complete freedom on how much he/she will give you, but you have complete freedom on how many beans to sort, once you learn what your boss has decided to give you for each gram of sorted beans.

Note that your boss will tell us his/her decision on how to split the 6 TZS, and we will give you the money then. Thus, you do not need to worry that your boss may not give you the money he/she is promising. After he/she made his/her decision, we will make sure that you get paid exactly according to this decision.

Do you have any questions?

Then let us see what your boss has decided to give you for each gram of beans you sort: [Play Round 2]

D.2.2. Script for ENTREPRENEURS

Now we start with the first part of the session today, in which we simulate a work-day through playing some simple games. Overall, you will play two games in this session, and it will take around 1 hours, together with all explanations. After that, we will do the interviews, and after the interview, you will receive the money that you will earn during the games, plus the 3000 TZS that you will get just for having shown up here today.

As we said earlier, in the games you will play the role of a BOSS. In each of the two games you will be matched with a different WORKER. Recall that WORKERS are those people that you and others who got invited to the sessions by staff from [show Logo] invited to come along. You will play round 1 with one worker, and round 2 with another worker. It is important to remember that the workers will not be the same in Round 1 and 2.

Some of you may be matched in one of the two rounds with the worker you invited yourself to come with you to the session today, while others will be matched in both Rounds with Workers that other Bosses brought along. Who is matched with the worker they brought along and who is matched with another worker is decided by a random lottery mechanism. It thus says nothing about you or your worker if you are matched with the worker you brought along, or with a different worker.

Round 2:

So let us look at what will happen in the first round of the game. The worker that you will be matched with will do a simple task for a period of 8 minutes. From a large bag with three types of beans, as you can see here, he/she has to sort beans of the same type into smaller plastic cups. He/She will get paid for each gram of beans of one type that has been sorted.

Here I show you the type of work-place that the worker will have, with a large sack of beans, and three empty small plastic cups in which he or she should put the beans. I give you a few minutes so that you can try out the task yourself.

[Give a few minutes for them to familiarize themselves with the task]

a. Addendum: Round 1, Group A: Baseline Game

Let us now see who you are matched with to go through the first round: [Reveal name of matched worker]

For you as a boss, there is very little you need to do during the first round. The worker we just showed you will do the simple task we just showed you. For each gram of sorted beans he/she sorts YOU will get 4 TZS, while the worker will get 2 TZS plus a fixed payment of 700 TZS. You do not have to do anything. Keep in mind, however, that the worker will be told that it is you who is his/her boss.

b. Addendum: Round 1, Group B: Boss does not know worker

For you as a boss, there is very little you need to do during this round. The worker that you are matched with will do the simple task we just showed you. For each gram of sorted beans he/she produces YOU will get 4 TZS, while the worker will get 2 TZS plus a fixed payment of 700 TZS. You do not have to do anything.

The crucial bit is that we cannot tell you who your worker is. This is an important element of this lab game. So all you need to do is sit back, relax, and wait till we tell you how much the worker you are matched with has produced, and therefore, how much you earned.

c. Addendum: Round 1, Group C: Boss Can "Tip"

Let us now see who you are matched with to go through the first round: [Reveal name of matched worker].

For you as a boss, there is very little you need to do during this round. The worker we just showed you will do the simple task we just showed you. For each gram of sorted beans he/she produces YOU will get 4 TZS, while the Worker will get 2 TZS plus a fixed payment of 700 TZS. You do not have to do anything. Keep in mind, however, that the worker will be told that it is you who is his/her boss.

The only thing that you will do is to decide if the worker should get an additional reward after he/she completes the task. For that, you will get an additional 1500 TZS after the round is over, and after you have learnt how many grams of beans the worker has correctly sorted, and how much money you will receive based on the amount of beans, you can decide how much of the 1500 TZS you want to give the worker as a reward, or a "Tip". You are totally free to decide how much to give him/her. It can be 0 if you want to, all of it, or any sum in between. Whatever of the 1500 TZS you decide not to give to the worker you can keep for yourself.

a. Addendum: Round 2, Group D: Boss sets Worker Rate

Let us now see who you are matched with to go through the first round: [Reveal name of matched worker].

For you as a boss, there is very little you need to do during this round. The worker we just showed you will carry out the simple task we described. Your only decision is how much he/she will get for each gram of sorted beans in addition to a fixed payment of 700 TZS he/she gets. For each gram of sorted beans, you will get 6 TZS. You need to decide, how many of these 6 TZS for each gram of sorted beans you want to pay to the worker. You keep the rest of the 6 TZS.

For example, you can decide to give 2 TZS to the worker for each gram of sorted beans. Then you will keep 4 TZS for each gram of sorted beans. Therefore, you decide how much you want to motivate the worker to work. It is plausible that the less money you give to the worker, the less motivated he/she is, and the less amount of sorted beans he/she will produce. But of course, the less money you give him/her for each gram of beans sorted, the more you will keep for each gram of sorted beans. But we cannot tell you how many beans the worker will sort for a given amount of money you are willing to give to him/her. That is a decision the worker makes on his/her own. Keeping that in mind, you have to decide how much to give to him/her.

Also keep in mind that we will pay the worker directly, according to the rate you set for him/her plus the fixed payment of 700 TZS. Thus, if you decided to give 2 TZS to the worker, we will give you 4 TZS, and 2 TZS directly to the worker for each gram of sorted beans plus a fixed payment of 700 TZS.

Let us try an example. Recall that you get 6 TZS for each gram of sorted beans. The worker produces. Now tell me how many of the 6 TZS you want to give to the worker for each gram of beans sorted. Note that this is not your final decision. So, you say you give.... [BOSS's response] TZS back for each gram of sorted beans. Now assume the

worker produces 200 grams. How many TZS … will you get in total? [Tick Y/N if answer is correct] … will the worker get in total? [Tick Y/N if answer is correct]

So tell me how many TZS you want to give to the worker for each gram of sorted beans out of the 6 TZS. This answer now counts! [ANSWER]

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